Letters

When Kin Correlations Are Not Squared

In response to a number of inquiries concerning the proportion of genetic variance in IQ explained by the MZA [monozygotic] correlation, we have prepared the following explanation (Articles, 12 Oct., p. 223).

It is a common misunderstanding that the intraclass correlation is squared to estimate the proportion of variance explained by genetic factors. Familial correlations represent components of variance; they are not squared (*1*).

The reason that the intraclass correlation is not squared in our application is that the quantity to be estimated is the proportion of variance in twin A's IQ that is associated with twin A's genotype, and not the proportion of variance in twin A's IQ associated with twins B's IQ. In the latter case, an observed intraclass of 0.70 would be squared to yield an estimate of 0.49 for the proportion of IQ variance shared by the two twins. In the former case, however, the observed phenotypes are imperfect indicators of the underlying genotypes, so that the correlation itself provides a direct estimate of the proportion of IQ variance shared with the unobserved genotype. The situation is analogous to the estimation of reliability in psychometrics whereby the correlation between two parallel forms of a test provides a direct estimate of the proportion of observed test score variance associated with unobserved true score variance (that is, the reliability of the test) (*2*).

THOMAS J. BOUCHARD, JR.
Department of Psychology, and Institute of Human Genetics, University of Minnesota, Minneapolis, MN 55455
DAVID T. LYKKEN
Department of Psychology, University of Minnesota, Minneapolis
MATTHEW MCGUE
Department of Psychology, and Institute of Human Genetics, University of Minnesota, Minneapolis
NANCY L. SEGAL
AUKE TELLEGEN
Department of Psychology, University of Minnesota, Minneapolis

REFERENCES AND NOTES

1. This result is best explicated by the use of a path diagram; see R. Plomin, J. C. DeFries, G. E. McClearn, *Behavioral Genetics: A Primer* (Freeman, New York, 1990), pp. 238–239.
2. This issue is also explicated by A. Jensen [*Psychol. Bull.* **75**, 223 (1971)] from the point of view of reliability theory and the common elements formula for correlation. See, also, the reply to Jensen by J. K. Miller and D. Levine, *ibid.* **79**, 142 (1973).

Frazil Ice

In the News & Comment article "Zebra mussel invasion threatens U.S. waters" by Leslie Roberts (21 Sept., p. 1371), reference is made to "frazzle" ice. "Frazil" is the correct spelling for the type of ice that blocked the Monroe, Michigan, water intake. This word, of French-Canadian origin, describes ice formed in turbulent, supercooled water. The term, from an Old French word meaning coal cinders (*fraisil*) apparently came into use because of the appearance of the ice.

STEVEN F. DALY
Ice Engineering Research Branch, Cold Regions Research and Engineering Laboratory, Corps of Engineers, Department of the Army, Hanover, NH 03755–1290

Imprisoned in Sudan

Moneim Attia, an eminent environmental physiologist from Khartoum, returned to Sudan some years ago after training and experience in Germany and Kuwait. His goal was to develop research on the problems raised by the local climatic challenges of his country. He was arrested in his home on the night of 13 January 1990. He has been detained without trial or accusation since then. We understand that his treatment has been inhumane in several ways, such as being kept without communication with his family, being frequently beaten, and being kept blindfolded day and night for long periods. His arrest was ordered by Lieutenant General Omar Hassan Al-Bashir, head of the Revolutionary Command Council for National Salvation, Khartoum, Sudan.

We the undersigned environmental physiologists urge our colleagues from all fields to write to Lieutenant General Al-Bashir, as well as to the ambassadors of Sudan in their countries, saying that they are aware of the bad treatment received by Moneim Attia and that this treatment (absence of trial or accusation, torture) violates several international conventions: (i) the Convention against Torture and Other Cruel, Inhuman or Degrading Treatment or Punishment; (ii) the Covenant of Civil and Political Rights; and (iii) the U.N. Body of Principles.

We understand that several other scientists are similarly detained in Sudan. What we do to defend Moneim Attia will have the general effect of helping protect all scientists who choose to help their countries.

H. BRINNEL
Pathophysiologie, L'Arbresle Hospital, 69593 L'Arbresle Cedex, France
M. CABANAC
Department of Physiology, Faculty of Medicine, Laval University, Quebec, G1K 7P4, Canada
R. HALES
School of Physiology and Pharmacology, University of New South Wales, Kensington (Sidney), New South Wales 2033, Australia
E. NADEL
John B. Pierce Laboratory, and *Yale University School of Medicine, 290 Congress Avenue, New Haven, CT 06519*
T. NAGASAKA
Department of Physiology, Faculty of Medicine, Kanazawa University, Kanazawa City, 920, Japan

Carcinogenesis Debate

In her News & Comment article discussing our papers on carcinogens (9 Nov., p. 743), Jean L. Marx says that our position is, "Below the toxic dose, carcinogenesis would not be a problem . . . because there would be no increased cell proliferation," that is, thresholds are the general case. That is not our view, as is clear from our papers. It is reasonable to assume that low levels of mutagens might add a small increment to our enormous endogenous level of DNA adducts coming from oxidant by-products of normal metabolism. However, the risk should be considerably lower than predicted by linear extrapolation from high dose tests because increases in mitogenesis can be unique to high doses and inducible general defense systems act as a buffer at low doses. The risk from nonmutagens at low doses may be zero (for example, in the case of saccharin). Our view, as can be seen in our papers, is not that mitogenesis is a single-factor explanation for carcinogenesis. Rather our view is that you cannot understand mutagenesis (and therefore carcinogenesis) without taking mitogenesis into account and that at high doses chronic mitogenesis can be the dominant factor. This is also the view of S. M. Cohen and L. B. Ellwein and is supported by their work (Articles, 31 Aug., p. 1007).

Numerous researchers have pointed out for years that chronic mitogenesis is important in carcinogenesis. Our theoretical point is that this is because of effects on mutagenesis. Loss of heterozygosity due to nondisjunction, gene conversion, and mitotic recombination occurring during cell division can be much more frequent than loss of heterozygosity due to an independent second mutation (*1*). Cell division is important in general for markedly increasing the probability of mutation and, for recessive genes, is likely to be of dominant importance after the occurrence of the first mutation.

Some of the other criticisms of our papers reported by Marx will be addressed in our responses to forthcoming letters in *Science*.

BRUCE N. AMES
Department of Molecular and Cell Biology, University of California, Berkeley, CA 94720
LOIS S. GOLD
Carcinogenic Potency Database, Lawrence Berkeley Laboratory, Berkeley, CA 94720

REFERENCES

1. B. N. Ames and L. S. Gold, *Proc. Natl. Acad. Sci. U.S.A.* **87**, 7772 (1990); R. Fahrig, *J. Cancer Res. Clin. Oncol.* **113**, 61 (1987); *Mol. Gen. Genet.* **194**, 7 (1984).

Primate Research Institute: AIDS Research Program

Karen Wright's article about changes at Primate Research Institute (PRI) (News & Comment, 2 Nov., p. 614) contains some statements and opinions attributed to me that distort the present status of research, in particular AIDS research, at PRI. A reevaluation of my conversation with Wright would reveal an upbeat tone and discussion of a healthy AIDS research program that was not conveyed by the article. Three statements from the article are germane.

1) "For the moment PRI certainly hasn't shut down. But it doesn't have much in the way of an AIDS research component." In fact, the truth is the exact opposite as I told Wright. Mika Popovic's group neither brought nor took AIDS research funds. No AIDS projects have been lost or impeded since Popovic, Suzanne Gartner, and Bill Hobson left PRI. Direct cost AIDS funding at PRI has grown this year from $2,475,051 to $3,080,713.

2) The article states that I have "not succeeded in filling any institutional vacancies. . . ." This is a distortion, as I told Wright that no positions had yet been listed or advertised. I had been at PRI only a few weeks when we spoke. Since then, PRI has had a professional position listed that has not yet closed. We have had several qualified applicants apply thus far.

3) The article states that when I was "asked if NMSU will cooperate by producing the financial or moral support needed to make PRI what he envisions," I hesitated and said "I don't know yet . . . I'm just beginning to find out." This statement appears to be worded to imply that I might be dissatisfied with support provided by the New Mexico State University (NMSU) administration. My answer was in reference to understanding the current financial situation at PRI and not to waiting for bailout funds, as implied. I stated very clearly that universities do not normally provide large amounts of intramural funds to primate centers. I also said that NMSU provided more indirect cost to PRI than I had experienced at the University of California, Davis, primate center.

When I became director of PRI in September 1990, I believed that the right changes had been made by the NMSU administration. Everything I have observed at PRI since then has confirmed that belief.

PRESTON A. MARX
Professor of Biology and Director, Primate Research Center, New Mexico State University, Holloman Air Force Base, NM 88330–1027

Response: Marx's optimism notwithstanding, I stand by my original article for the following reasons: (i) The AIDS research component my article refers to is the on-site virology-immunology group conceived by the institute's former director—a group that has dissolved. Therefore the level of PRI's *ongoing* funding for chimps used by AIDS researchers at *other* institutions seems irrelevant. (ii) Marx says he had not advertised for replacements for Popovic *et al.* at the time of our interview, but that has little to do with the phrase he quotes from my article saying that he had not yet filled any vacancies. After all, that phrase was still true at press time—well after the interview—and indeed appears from Marx's own letter to have been still true when he penned his critique. (iii) Marx states that my article incorrectly implies that he was "dissatisfied with support provided by the NMSU administration." But in the sentence following the one he quotes above, I wrote: "Marx says he doesn't expect NMSU to subsidize his ambition." Similarly, I later quote Marx as saying that NMSU's support for PRI compares favorably with other universities' support for their primate centers.—KAREN WRIGHT